

The direct and spillover effects of a nationwide socio-emotional learning program for disruptive students.*

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Abstract

Social and emotional learning (SEL) programs teach disruptive students to improve their classroom behavior. Small-scale programs in high-income countries have been shown to improve treated students' behavior and academic outcomes. Using a randomized experiment, we show that a nationwide SEL program in Chile has no effect on eligible students. We find evidence that very disruptive students may hamper the program's effectiveness. ADHD, a disorder correlated with disruptiveness, is much more prevalent in Chile than in high-income countries, so very disruptive students may be more present in Chile than in the contexts where SEL programs have been shown to work.

Keywords: disruptive students, spillover effects, peer effects, social and emotional learning.

JEL Codes: I21, I24, I28, D62.

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1 Introduction

Lazear [2001] has proposed that classroom learning is a public good suffering from congestion effects, which are negative externalities created when one student is disruptive and impedes the learning of her classmates. In the US, those externalities are important: Carrell and Hoekstra [2010] and Carrell et al. [2018] find that being exposed to one peer experiencing domestic violence at home, a good proxy for a disruptive peer, reduces classmates' test scores by 0.07 standard deviation (σ), and reduces their earnings at age 26 by 3 to 4 percent. Figlio [2007] also finds that being exposed to disruptive peers reduces classmates test scores. Betts and Shkolnik [1999] find that US middle and high schools teachers devote 6.1% of instruction time to discipline, and that this fraction is higher in disadvantaged schools. Therefore, programs effective at reducing troubled students' disruptiveness may generate large positive spillover on their classmates, on top of their direct effects.

Epidemiological studies show that the prevalence of ADHD, a disorder correlated with conduct problems, is higher in some low- and middle-income countries than in high-income countries. Then, addressing students' conduct problems may be an even more pressing issue in those countries. In Chile, the country where the intervention we study takes place, 15.5% of primary school children have ADHD (see [de la Barra et al., 2013]). Primary school children also have been found to have high ADHD rates in Colombia (16.9%, see [Cornejo et al., 2005]), or in Iran (17.3%, see [Safavi et al., 2016]). On the other hand, the ADHD prevalence rate among primary school children is estimated at 6.8% in the US (see [Visser et al., 2014]), between 3.5 and 5.6% in France (see [Lecendreux et al., 2011]), and 3% in Italy (see [Bianchini et al., 2013]).

School-based mental health programs are often used to reduce students' disruptiveness. Some programs are universal, meaning that they are delivered in classroom settings to all the students in the class. Other programs are selected, meaning that they are provided to students identified by teachers as having conduct problems, during the school day and outside the classroom. Many school-based mental health programs are social and emotional learning (SEL) programs (see [Wilson and Lipsey, 2007]), that teach children to recognize and manage their emotions, and to handle interpersonal situations effectively, using cognitive and behavioral therapy (CBT). A vast literature has found SEL programs to be successful. In a meta-analysis of 80 selected interventions, Payton et al. [2008] find that they reduce conduct problems by 0.47σ , and respectively improve mental health and academic performance by 0.50 and 0.43σ . Meta-analyses of universal SEL interventions find smaller but still large effects on those dimensions, around -0.25σ for con-

duct problems, and $+0.55\sigma$ and $+0.30\sigma$ for mental health and test scores ([Durlak et al., 2011], [Sklad et al., 2012], [Wigelsworth et al., 2016], [Taylor et al., 2017], and [Corcoran et al., 2018]).

However, there are at least two gaps in the literature. First, it has mostly considered small-scale demonstration programs mounted by researchers in a handful of schools. The effect of SEL interventions may differ when implemented at scale (see [Davis et al., 2017] or [Weisz et al., 2014]). Second, it has mostly focused on interventions conducted in high-income countries, while epidemiological studies suggest that addressing students’ conduct problems may be more pressing in some middle- and low-income countries. A recent meta-analysis of psycho-social interventions for disruptive students in low- and middle-income countries (see [Burkey et al., 2018]) includes only two SEL interventions, one in Jamaica and the other in Romania. Both are universal interventions, implemented at a very small scale. Both find positive effects on students’ conduct problems (see [Baker-Henningham et al., 2009] and [Ştefan and Miclea, 2013]).

This paper contributes to addressing those two gaps: it is the first to measure the effects of a nationwide SEL program, and the program we study takes place in a middle-income country with a high ADHD prevalence rate. Specifically, we study the effects of “Skills for Life” (SFL), a selected SEL program for disruptive second graders in Chile. Since its creation in 1998, SFL has screened and treated around 1,000,000 children, making it the fifth largest school-based mental health program in the world (see [Murphy et al., 2017]). To identify eligible students, SFL teams use a psychometric scale measuring students’ disruptiveness, and students above some cut-off are eligible. Eligible students then follow 10 two-hours SEL sessions with a psychologist and a social worker. SFL is a costly program: we estimate that its cost per student is equivalent to 15% of the expenditure per primary school student in Chile.

We randomly assigned 172 classes to either receive SFL in the first or second semester of the 2015 school year, and we measured outcomes at the start of the second semester, after the treatment group had received the treatment but before the control group received it. By comparing eligible students in the treatment and control groups, we can estimate the direct effects of the program, and by comparing ineligible students in the two groups we can estimate its spillover effects.

We find that SFL does not have effects on eligible students’ disruptiveness, mental health, and academic achievement. The effects we can rule out are fairly small, and much smaller than those found in Payton et al. [2008] and in all the other SEL meta-analyses we are aware of. For instance, we can rule out at the 5% level that the program increases students’ Spanish scores by more than

0.09σ , or that it reduces teachers' assessment of students' disruptiveness by more than 0.10σ . Not surprisingly, as we do not find that SFL impacts eligible students, we also do not find spillover effects on ineligible ones. Finally, we even find that the program has a strong negative effect on teachers' and enumerators' ratings of the overall disruptiveness of treated classes.

To account for the discrepancy between our results and the literature, we compared SFL to the selected SEL interventions reviewed in Payton et al. [2008]. Three conclusions emerge. First, SFL's intensity (number of sessions, duration...) is comparable to that of the meta-analysis's interventions, so it is not the case that SFL is not intensive enough to produce an effect. Second, as ADHD is much more prevalent in Chile than in high-income countries, SFL may be faced with a harder-to-treat population than the interventions reviewed in Payton et al. [2008]. We indeed find evidence that SFL's effectiveness is hampered by the presence of very disruptive students. In classes with at least one very disruptive eligible student, defined as students above eligible students' 90th percentile of baseline disruptiveness,¹ the program increases the disruptiveness of other eligible students, of ineligible students, and it worsens teachers' and enumerators' ratings of the overall class disruptiveness. The program also strongly increases the friendship ties between very disruptive and other eligible students. The former may then have a negative influence on the latter, which would explain the negative effects we observe. Third, SFL and the meta-analysis's interventions strikingly differ in terms of scale and delivery. The interventions in the meta-analysis are demonstration programs mounted by researchers, that typically treat a few dozens children in a handful of schools. Half are delivered by the researchers, while the other half are delivered by psychologists or teachers under researchers' close supervision: typically, researchers review their delivery of the intervention every week. On the other hand, SFL is a large-scale governmental program, delivered by psychologists without any researcher involvement. The governmental agency in charge of the program loosely monitors the program implementers, and very rarely audits their workshops. Without sufficient monitoring, teams may not implement the program with high-enough fidelity, which could also explain why SFL does not produce an effect.

The remainder of the paper is organized as follows. In Section 2, we present the SFL program. In Section 3, we present the randomization, the data we use, and the population under study. In Section 4, we present compliance with randomization, the balancing checks, and attrition. In Section 5, we present the main results. In Section 6, we interpret the results and present some exploratory analysis.

¹This definition ensures that about 50% of classes have at least one very disruptive student.

2 The SFL program

SEL is the process through which children acquire the skills to recognize and manage their emotions, set and achieve positive goals, and handle interpersonal situations effectively. SEL programs try to enhance children’s self-awareness (accurately assessing one’s feelings and maintaining a sense of self-confidence), self-management (regulating one’s emotions and controlling impulses), and social awareness (being able to take the perspective of others, preventing, managing, and resolving interpersonal conflict). Selected programs are provided to specific students identified as having conduct problems, during the school day and outside of their classroom. Meta-analysis have shown that selected SEL programs improve SEL skills, reduce conduct problems, and can improve academic achievement (see [Payton et al., 2008]).

SFL is a Chilean school-based selected SEL program for second graders suffering from conduct disorders. It is managed by JUNAEB (*Junta Nacional de Auxilio Escolar y Becas*), the division of the Chilean Department of Education in charge of most of the non-teaching programs implemented in Chilean schools. The program started as a pilot in 1998. Over the next 3 years, JUNAEB collaborated with psychologists from the University of Chile to review the screening measures and programs available at that time, and design an SEL program adapted to Chile, where the prevalence of ADHD is particularly high among children (see [de la Barra et al., 2013]). The program became a nationwide policy in 2001, and it is currently implemented in 1,637 publicly-funded elementary schools in Chile (see [Guzmán et al., 2015]). These schools account for 20% of all elementary schools in Chile, and they are the most disadvantaged. Since 1998, the SFL program has screened and treated around 1,000,000 children, making it the fifth largest school-based mental health program in the world (see [Murphy et al., 2017]).

To identify eligible students, SFL uses a psychometric scale, the Teacher Observation of Classroom Adaptation (TOCA, see [Kellam et al., 1977], and [Werthamer-Larsson et al., 1990]), adapted to the Chilean context by George et al. [1994]. In the end of each academic year, first-grade teachers answer the TOCA questionnaire for each of their student. Based on this questionnaire, students receive scores on the following six scales: authority acceptance (AA), attention and focus (AF), activity levels (AL), social contact (SC), motivation for schooling (MS), and emotional maturity (EM). The TOCA questionnaire concludes with two summary questions, where teachers have to give ratings of the overall disruptiveness and academic ability of each of their student.

Then, the three following groups of students are eligible for the program:

- Students above the 75th percentile of the AA scale, above the 85th percentile of the AF and AL scales, and below the 25th percentile of the MS scale;
- Students below the 25th percentile of the SC scale, and either above the 75th percentile of the AA scale or above the 85th percentile of the AL scale;
- Students below the 25th percentile of the SC, MS, and EM scales, and below the 50th percentile of either the AA or AL scale.

The percentiles are gender specific, to ensure that not only males are eligible, and were computed using a representative sample of the 2nd grade population in Chile (see [George et al., 1994, De La Barra et al., 2005]). Students in the third eligibility group are not disruptive, but they only account for 7% of eligible students, while the first two groups respectively account for 40% and 53% of eligible students. Depending on the year, eligible students account for 15 to 20% of first-grade students whose teachers answer the TOCA questionnaire.

In second grade, SFL asks eligible students’ parents the authorization to enroll their child in the program. If their parents accept, eligible students are enrolled in a workshop implemented by a team of two SFL employees. A survey conducted in 2015 (see [Rojas-Andrade and Leiva, 2018]) shows that half of SFL employees are psychologists. In Chile, this title can be obtained after a college degree with a psychology major (see [Guzmán et al., 2015]). The other half of employees are social workers and former teachers, titles that can also be obtained after a college degree. Usually, an SFL team consists of a psychologist and a social worker or teacher. 77% of SFL employees are women, their average age is 31 years old. They have on average 2.6 years of experience into the program, and 36% have less than one year of experience, indicating a high rate of turnover. During their first year, SFL employees receive three eight-hours-long days of training (see [Rojas-Andrade et al., 2017]). They also attend “good practices” meetings every six months, in which they share with other teams what works in their workshops. As the Chilean public school system is administrated at the municipal level, SFL teams are also organized at this administrative level.

SFL workshops consist in 10 two-hours group sessions, taking place weekly, during the class day, over the course of one semester. During sessions, enrolled students leave the classroom, while their classmates stay there and continue with their normal schedule. The time of the group sessions is set in coordination with teachers, to avoid that enrolled students lose key instruction time. During the workshops, teachers teach subjects deemed less crucial than Spanish or mathematics, like religion (a mandatory subject in Chile) or music, to the ineligible students that stay with them

in the classroom (see [Rojas-Andrade and Leiva, 2018]). The workshop takes place over two school periods, and eligible students come back to their classroom before the break.

Sessions are divided into five parts. The goals of the first part are to welcome children and build a group identity, for instance by having children choose a group name. The goals of the second part are to improve children’s self-esteem, and their respect of others. Then, during the third part, the psychologists help students put words on their and others’ emotions, and help them share their emotions with others. Then, the fourth part is dedicated to self-control techniques, and to strategies to find non-violent solutions to conflicts. Finally, the last part is dedicated to a review of what has been learnt during the workshop. Sessions are activity based, involve games and role play, and make use of CBT techniques. If they behave well during a session, students sometimes receive rewards like cakes or candies. SFL employees are provided with a 114-pages-long manual describing the goal and the content of each session, and suggesting games and activities. But they are also encouraged to tailor the content of their sessions to the specific needs of the students enrolled.

As per the SFL guidelines, six to 12 students should participate in a workshop. If there are less than six eligible students in a school, no workshop takes place, and if a school has more than 12 eligible students, two workshops take place in that school. In the next section, we explain how we exploit these features in our randomization. Finally, the parents of enrolled children are invited to three training sessions, whose goal is to encourage them to reproduce the workshop’s activities at home.

We estimate that SFL costs 200 USD per treated student. We also estimate that the government spends 1,316 USD on instruction per student and per year in the schools in our sample.² Therefore, the program’s cost represents a sizeable 15% increase of the expenditure per student. JUNAEB does not have an estimate of the total cost of the program, here is how we estimated it. The 2014 budget of one of the municipal teams in our sample shows that its program implementers earned on average 7.42 USD per hour in 2014. Then, based on interviews with two implementers, we estimated that it takes 149 hours of work to implement an SFL workshop. This includes the 52 hours that the two workshop implementers spend delivering 13 two-hours sessions to students and their parents, but also the time that they spend: preparing the sessions and buying the ma-

²The government funds public schools by giving them a voucher per student, whose amount depends on the student’s attendance (<https://www.oecd-ilibrary.org/docserver/9789264287112-6-es.pdf?>). For public primary schools, the school voucher is worth 754 USD for an attendance of 84%, the average attendance observed in our sample. Then, the government gives schools an extra voucher worth 721 USD for every very disadvantaged student (https://ate.mineduc.cl/usuarios/admin3/doc/2015020312570909985.Manual_Apoyo_a_la_Gestion.pdf), and 78% of students are very disadvantaged in the schools we study, thus leading to our $754+0.78\times 721=1,316$ USD estimate.

material they need; going to and returning from the school for each session; preparing the reporting documents JUNAEB asks them to send for each workshop; meeting with the school principal and 2nd grade teachers prior to the start of the workshop, to agree on the schedule and location of the workshop; and interview 1st grade teachers to fill the TOCA questionnaire for each of their students the year before the workshop. Then, the team’s budget shows that digitizing the 2014 TOCAs of all the first grade students in the town costed 860 USD. Divided by the 10 workshops conducted that year, that leads to a cost of 86 USD per workshop. Implementers also received transportation vouchers worth 63 USD per workshop. Finally, the cost of the material needed for the workshop activities is estimated at 188 USD per workshop, based on a detailed list of all the items bought for a workshop provided by the implementers we interviewed. Overall, we estimate the total cost of a workshop at $7.42 \times 149 + 86 + 63 + 188 = 1,443$ USD. The team whose budget we used had 7.2 students per workshop in 2014, which finally yields our estimated cost of 200 USD per treated student. This estimate relies on one team’s budget. Costs may vary between teams, but we do not have reasons to suspect that the program’s average cost is orders of magnitude away from our estimate.

Previous research has found that from first to third grade, the disruptiveness of students that attend seven to 10 SFL sessions in second grade decreases more than that of students attending six sessions or less (see e.g. [Guzmán et al., 2015]). However, SFL attendance is driven by students’ school attendance, and students who attend school less may do so because they experience negative shocks, which could explain why their disruptiveness decreases less. To avoid that type of endogeneity bias, our paper relies on an experimental control group to measure the effect of SFL.

3 Randomization, data, and study population

3.1 Sample selection and randomization

Our sample consists of 172 classes. All municipal teams conducting the SFL program in the Santiago and Valparaiso regions, the two most populated regions in Chile, were invited to join the study. 32 out of 39 accepted our invitation. In March 2015, these teams visited the schools covered by the program in their municipalities, and collected data on the number of students eligible for the program enrolled in each second grade class. 172 classes with four or more eligible students and in schools with six or more eligible students were included in the study. The second criterion ensured that group sessions would indeed take place in the school, while the first criterion ensured that there were enough treated students per class to potentially generate spillover effects. About

450 classes participate in a SFL workshop each year in the Santiago and Valparaiso regions, so our sample covers about 40% of the classes covered by the program in those regions.

Randomization took place both within schools and within municipalities. There were 29 schools with two classes included in our sample and where it was possible to form two groups of six students or more without grouping students of the two classes together. In such instances, we conducted a lottery within the school, to assign one of the two classes to receive the treatment in the first semester of 2015, and the second class to receive it in the second semester. The remaining 114 schools each only had one class included in our sample, so randomization took place within municipalities. In this latter group of schools, there is no risk that the control group students may have been contaminated by the treatment, while this may have happened in the former group of schools. Later in the paper, we reestimate the treatment effect in the second group of schools and find very similar effects to those we find in the full sample, so control-group contamination does not seem to drive our results.

Overall, we conducted 56 lotteries (29 within schools, and 27 within municipalities) and we assigned 89 classes to receive the treatment in the first semester, from April to June 2015, and 83 to receive it in the second semester, from September to December 2015.

3.2 Data

In our analysis, we use data produced by JUNAEB. First, we use the six first-grade TOCA scores that determine students' eligibility to SFL, as well as the teachers' ratings of students' disruptiveness and academic ability in the TOCA questionnaire. Then, we also use another psychometric scale collected by JUNAEB and measuring students' disruptiveness, the pediatric symptom checklist (PSC, see [Jellinek et al., 1988]), which is filled by students' parents. We also use JUNAEB's data on treatment implementation. Specifically, for each class in our sample we know how many SFL group sessions were conducted in the first semester of 2015. For each student, we know how many sessions she attended, and how many sessions her parents attended. Finally, JUNAEB also provided us data on students' socio-economic background, as well as their monthly school attendance from March 2015 to June 2015.

We also use baseline data collected in March 2015, before the treatment started in the treatment group classes, and endline data collected in August 2015, after the treatment ended in the treatment group classes and before it started in the control group classes. Both at baseline and endline, two enumerators visited each of the 172 classes included in the experiment during a half day. Enumerators were undergraduate students, mostly psychology and education majors. Every

person who applied to become an enumerator first had to attend a half-day training, during which he/she was taught how to administer our questionnaires. Candidates also had to take a test at the end of the training, and only those who scored above some threshold became enumerators.

Our questionnaires slightly changed from baseline to endline. Below, we describe our endline questionnaires, and we explain the difference between our baseline and endline questionnaires when needed later in the paper.

The enumerators first administered a non-cognitive questionnaire to the students. That questionnaire aimed at measuring:

- Students' happiness in school, using a question from the student SIMCE questionnaire.³
- Students' self-control, using items of the child self-control psychometric scale (see [Rorhbeck et al., 1991]) that we translated into Spanish.
- Students' self-esteem, using items of the self-perception for children psychometric scale (see [Harter, 1985]) translated and validated into Spanish (see [Molina et al., 2011]).

Second, the enumerators administered a Spanish and mathematics test to the students. Third, the enumerators interviewed individually each student and asked her to name up to three students that she likes to play with during breaks, hereafter referred to as the student's friends. Fourth, the enumerators observed a one-hour lecture. During that observation, they observed the behaviour of each student during five seconds, and assessed whether the student was studying, not studying, or being disruptive. They repeated that process five times, and then rated the overall disruptiveness of each student by answering the summary question from the TOCA questionnaire. During that one-hour lecture, the enumerators also recorded the decibel levels in the class using a smartphone app, and wrote down the time at which the lecture was supposed to start and the time when it effectively started. Fifth, the enumerators filled a short questionnaire aimed at assessing the overall disruptiveness in the class, using questions taken from the PISA (Program for International Student Assessment) questionnaire, asking them their agreement with statements such as: "There is noise and disorder in this class," or "The teacher has to wait for a long time before students calm down and he/she can start teaching".

³The SIMCE (*Sistema de Medición de la Calidad de la Educación*) questionnaires are the nationwide standardized cognitive and non cognitive questionnaires administered to students and teachers in Chile.

The enumerators also administered a questionnaire to the teachers. That questionnaire aimed at collecting: teachers' socio-demographic characteristics; teachers' ratings of the overall disruptiveness of the class, using similar questions as those asked to enumerators; teachers' rating of the prevalence of bullying in the class; teachers' motivation, taste for their job, and mental health levels. The questionnaire was for the most part composed of questions from the SIMCE teacher questionnaire. Teachers also rated the overall disruptiveness of each of their student by answering the summary question from the TOCA questionnaire.

Finally, in July 2019 we also conducted qualitative interviews to shed light on the mechanisms underlying our results. We interviewed three of the SFL municipal teams that had participated in our experiment, and that account for 12% of our sample.

The list of the outcome variables we consider in the paper was pre-specified in a pre-analysis plan (PAP) available at <https://www.socialscienceregistry.org/trials/1080>. That plan was time-stamped on 04/28/2017, before JUNAEB sent us students' first grade TOCA scores, as a letter from JUNAEB officials also available on the social science registry website testifies. Students' first-grade TOCA scores are necessary to distinguish eligible and ineligible students in our data, a distinction that underlies most of our analysis. Even though endline took place almost two years before we submitted our PAP, we had not started to analyze our data before. Indeed, we had not finished cleaning the data before submitting our PAP. This research was funded through four small grants, totalling 37,000 GBP. Therefore, we could not afford to buy tablets to collect our data, and instead used paper questionnaires. We could also not afford to hire a RA in charge of supervising data entry and data cleaning. Instead, we supervised the RAs in charge of data entry ourselves, and we also took care of the data cleaning ourselves. This process ended after 04/28/2017.

The analysis presented in Sections 4 and 5 follows our pre-analysis plan, except for a few exceptions described below. On the other hand, the analysis presented in Section 6 was not pre-specified in our PAP. The student-level outcome measures listed in our PAP are:

- the student's happiness in school, self-control, self-esteem, Spanish, and mathematics scores,
- the percentage of school days missed by the student from April to June 2015,
- the rating of the student's disruptiveness by her teacher,
- the average rating of the student's disruptiveness across the two enumerators,

- the percentage of the student’s classmates that nominate her as one of their friends,
- an indicator for whether the student is not nominated as a friend by any other student,
- the average disruptiveness at baseline of the student’s endline friends,
- the average baseline Spanish and mathematics scores of the student’s endline friends.

The class-level outcome measures listed in our PAP are:

- the teacher’s rating of the class’s disruptiveness, constructed using teachers’ answers to the PISA questions measuring the disruptiveness in the class,
- the teacher’s rating of the prevalence of bullying in the class,
- the average rating of the class’s disruptiveness across the two enumerators, constructed using enumerators’ answers to the PISA questions measuring the disruptiveness in the class,
- the number of minutes between the moment the class was supposed to start and the moment it effectively started according to the enumerators,
- the average decibel levels during the class across the two enumerators’ recordings.

We standardize the school happiness, self-control, self-esteem, disruptiveness and test score measures to have a mean of 0 and a σ of 1 in the sample.

3.3 Assessing data quality

Some of the dimensions we are trying to measure are hard to observe. To get a sense of the reliability of our measures, Table A1 shows their baseline-endline correlation in the control group. Students’ Spanish and mathematics test scores have high positive baseline-endline correlations, above 0.5. Those correlations are still far from one, probably because students in our study are young and their cognitive ability is not fixed yet. Our measure of students’ popularity has a baseline-endline correlation of 0.32. Our school happiness, self-esteem, and self-control measures respectively have baseline-endline correlations of 0.22, 0.13, and 0.14.

Turning to disruptiveness measures, the rating of students’ disruptiveness by teachers has a baseline-endline correlation of 0.42, which is almost as high as the baseline-endline correlation of test scores. This is all the more remarkable as we use first grade teachers’ answer to the TOCA summary

question as our baseline measure,⁴ so our baseline and endline measures were not made by the same teacher. This suggests that students' disruptiveness is relatively stable, and that different teachers tend to agree in their ratings. Then, Table A2 shows that this measure is negatively correlated with students' academic ability: at baseline, its correlation with students' average test score in Spanish and mathematics is equal to -0.28. Finally, the bottom panel of Table A1 shows that teachers' rating of the disruptiveness of the class also has a high baseline-endline correlation, equal to 0.50.

In our PAP, we had planned to use the average of the two enumerators' ratings of a student's disruptiveness as our enumerator disruptiveness rating. However, this measure has a baseline-endline correlation close to, and insignificantly different from, zero. This could be due to the fact that endline and baseline observations are made by different enumerators, who may have different standards to assign a given grade on the disruptiveness scale. Therefore, we depart from our PAP, and slightly modify our measure. We start by regressing enumerators' ratings on enumerator fixed effects, in the sample of control group classes. Then, we compute the residuals from that regression both for treatment and control group classes, and we use the average of those residuals, across the two enumerators that have rated a student, as our enumerators' rating. This modified measure is the difference between a student's average rating by the two enumerators and the average of the ratings made by the same enumerators in the control group. Panel A of Table A1 shows that it has a positive and significant baseline-endline correlation equal to 0.13, and Panel A of Table A2 shows that it correlates well with teachers' ratings, and reasonably well with students' academic ability. Overall, enumerators' ratings of students' disruptiveness seem noisier than teachers', but they are still meaningful. Then, Panel B of Table A1 shows that enumerators' ratings of classes' disruptiveness have a relatively high baseline-endline correlation, around 0.25, and Panel B of Table A2 shows that this measure correlates well with teachers' ratings. Contrary to teachers' ratings, enumerators' ratings are blinded: enumerators do not know if the class they observe has been treated or not.⁵

The decibel measure constructed following our PAP also has a very low baseline-endline correlation, and it does not correlate at all with teachers' and enumerators' ratings of classes' disruptiveness. The app's measurement does not seem very precise: enumerators recording the same lecture sometimes end up with average noise levels differing by more than 10 decibels. This measurement also seems to depend on the make of the phone and on idiosyncratic factors specific to the enumerator's phone. Therefore, we depart again from our PAP, and net out enumerators' fixed effects from

⁴We decided to include the summary TOCA question in our baseline teacher questionnaire after having collected more than half of the baseline data, so that variable is missing for many classes at baseline.

⁵Previous literature on SEL interventions has also relied on non-blinded teacher ratings (see [Payton et al., 2008]).

decibel measures, exactly as we did for enumerators’ disruptiveness ratings. This new measure has a higher baseline-endline correlation than the measure described in our PAP, though Table A1 shows that this correlation is still not significant. But it also has a much larger correlation with enumerators’ ratings of the class disruptiveness, and that correlation is significant as shown in Table A2.

3.4 Study population

The 172 classes included in our sample bear 5,704 students, meaning that classes have an average of 33.2 students. 4,466 students are ineligible to the program (26.0 per class), while 1,238 students are eligible (7.2 per class). Column (1) in Table 1 below presents the baseline characteristics of ineligible students. 33.8% of them are born to teenage mothers, which is more than twice the corresponding proportion in Chile.⁶ 75.2% of them live in households below the 20th percentile of the social security score. Being below this threshold opens eligibility for 22 social programs and is usually considered as a proxy for poverty. 44.4% of them live in households below the 5th percentile of the social security score. Being below this threshold opens eligibility for 3 more social programs and is usually considered as a proxy for extreme poverty. Overall, the students included in our study live in households disproportionately coming from the bottom of the Chilean income distribution.

Column (2) in Table 1 presents the baseline characteristics of eligible students, and Column (3) reports the p-value of tests that the baseline characteristics of eligible and ineligible students are equal. Panel A shows that eligible students are more likely to be males and less likely to live with their father. Their parents are also less educated than that of ineligible students. Panel B shows that eligible students’s self-control and self-esteem scores are about 0.2σ lower than that of ineligible students. Differences are even more pronounced when one considers students’ disruptiveness and academic ability. Eligible students score 1.2σ higher than ineligible students on first-grade teachers’ disruptiveness ratings, and 0.4σ higher on enumerators’ baseline ratings. They also score 0.4σ lower on the Spanish and mathematics tests. Eligible students are also less popular than ineligible ones: 7.6% of the students in the class nominate them as friends, against 8.8% for ineligible students. The average disruptiveness of their friends is also about 0.2σ higher than that of ineligible’s friends, thus suggesting some assortative matching along the disruptiveness dimension.

Finally, Table A4 shows some characteristics of the teachers in our sample. 96.3% of teachers are females. Their average age is 42.8 years old, they have an average of 16.5 years of experience as a teacher, and 8.6 years of experience in the school where they currently teach.

⁶See <http://web.minsal.cl/portal/url/item/c908a2010f2e7dafe040010164010db3.pdf>.

Table 1: Characteristics of eligible and ineligible students

	Ineligible (1)	Eligible (2)	P-value (3)	N (4)
Panel A: demographic characteristics				
Male	0.498	0.582	0.000	5704
Teen mother	0.338	0.36	0.199	4440
Student lives with father	0.635	0.554	0.000	3765
\leq p20 social security score	0.752	0.77	0.198	5068
\leq p5 social security score	0.444	0.456	0.469	5068
Mother’s education	9.131	8.564	0.000	4727
Father’s education	9.163	8.439	0.000	4117
Panel B: baseline measures				
School happiness score	0.023	-0.063	0.022	4431
Self-control score	0.048	-0.166	0.000	4594
Self-esteem score	0.041	-0.146	0.000	4610
Overall disruptiveness TOCA	-0.293	0.873	0.000	4850
Disruptiveness, enumerator	-0.03	0.341	0.000	4645
Spanish test score	0.095	-0.335	0.000	4758
Math test score	0.082	-0.289	0.000	4758
% class friends with student	0.088	0.076	0.000	4721
Friends’ average disruptiveness	-0.051	0.188	0.000	3931

Notes: This table reports descriptive statistics for students in the sample. Column (1) reports the mean of the outcome variable for ineligible students and Column (2) reports the mean of the outcome variable for eligible students. Column (3) reports the p-value of a test that the two means are equal. Column (4) reports the number of observations used in the comparison.

4 Compliance, internal validity, and estimation methods

4.1 Compliance with randomization and fidelity of treatment assignment

In this section, we show that the SFL teams followed the randomization, and implemented the treatment as per the program’s rules: in the treatment group classes, very few ineligible students received the program. To do so, we estimate the effect of being assigned to treatment on actual exposure to treatment during the first semester of 2015. Let Y_{ijk} be a measure of exposure to treatment for student i in class j and lottery k . We estimate the following regression:

$$Y_{ijk} = \gamma_k + \beta D_{jk} + u_{ijk}, \quad (1)$$

where the γ_k s are fixed effects for the 56 lotteries we conducted to assign the treatment, and where D_{jk} is equal to 1 if lottery k assigned class j to the treatment group and to 0 otherwise. $\hat{\beta}$ estimates

a weighted average across lotteries of the within-lottery difference between the average of Y_{ijk} in treatment and control group classes. As our lotteries have few classes, the treatments of classes in the same lottery are strongly negatively correlated. Therefore, we cluster standard errors at the lottery level, following the recommendation of [de Chaisemartin and Ramirez-Cuellar, 2019]), who show that clustering at the class level could lead to substantial over-rejection of the null hypothesis.

To estimate the effect of assignment to treatment on class-level measures of exposure, we estimate Regression (1), except that we use propensity score reweighting instead of lottery fixed effects. With propensity score reweighting, β is also identified out of comparisons of treatment and control group classes in the same lottery (see [Hirano et al., 2003]). Using propensity score reweighting ensures that the regression does not have too many independent variables with respect to its number of observations (with lottery fixed effects, Regression (1) would have 57 independent variables and at most 172 observations). In any case, as the share of treated classes is equal to 0.5 in 46 of the 56 lotteries, using lottery fixed effects or propensity score reweighting does not make a large difference.

Column (1) of Table 2 below shows the mean value of eight measures of exposure to the treatment in the control group. Column (2) shows estimates of β for these eight measures. Column (3) shows estimates of the standard error of $\hat{\beta}$. Column (4) shows the p-value of a t-test of $\beta = 0$. To account for the fact that we consider several measures of exposure to the treatment, Column (5) shows the p-value controlling the False Discovery Rate (FDR) across the eight tests (see [Benjamini and Hochberg, 1995]). Finally, Column (6) shows the number of observations used in the estimation.

Panel A of the table shows that SFL sessions were conducted in 8.4% of the control group classes and in 98.1% of the treatment group classes. On average, 0.6 sessions were conducted in the control group classes against 9.5 in the treatment group classes. Throughout the paper, we estimate intention to treat (ITT) effects of assigning a class to the treatment. Given that less than 10% of the control group classes received the treatment, while almost 100% of treatment group classes received it, this ITT effect “almost” estimates the effect of delivering the treatment in a class.

Panel A also shows that 4.8% of eligible students in the control group attended at least one session, against 84.9% in the treatment group. Some eligible students did not attend any group session, either because their parents refused that they participate, or forgot to send back the document they had to sign to authorize their child’s participation. Table A3 compares the characteristics of the “takers”, eligible students in the treatment group that attended at least one session, to those of the

“non takers” that did not attend any session. The main difference between the two groups is that the takers are less disruptive at baseline. On average, eligible students attended 0.4 sessions in the control group, against 7.4 in the treatment group. This number is 8% lower than $9.5 \times 0.849 = 8.1$, the number we would have observed if students attending at least one session had attended all the sessions conducted in their class. This small difference is due to the fact that those students sometimes miss school on a workshop day, but school absenteeism does not seem to reduce students’ exposure to the program very much. Finally, Panel A shows that the fidelity with the program’s assignment rules was very high: in the treatment group, only 1% of ineligible students attended at least one session.

Panel B of the table shows that compliance with randomization was lower for the parents’ than for the students’ workshops: 53.5% of eligible parents in the treatment group attended at least one session, and eligible parents attend on average 1.0 sessions out of 3.

Table 2: Compliance with randomization

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: students’ workshops						
≥ 1 session conducted in class	0.084	0.897	0.035	0.000	0.000	172
Sessions conducted in class	0.602	8.942	0.337	0.000	0.000	172
Eligible students attended ≥ 1 session	0.048	0.801	0.029	0.000	0.000	1238
Sessions attended by eligible students	0.37	6.992	0.304	0.000	0.000	1238
Ineligible students attended ≥ 1 session	0.000	0.01	0.004	0.011	0.016	4466
Sessions attended by ineligible students	0.000	0.089	0.038	0.022	0.028	4466
Panel B: parents’ workshops						
Eligible parents attended ≥ 1 ses.	0.048	0.487	0.039	0.000	0.000	1238
Sessions attended by eligible parents	0.099	0.933	0.107	0.000	0.000	1238
Ineligible parents attended ≥ 1 ses.	0.000	0.008	0.004	0.039	0.043	4466
Sessions attended by ineligible parents	0.000	0.016	0.008	0.062	0.062	4466

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator. For student-level dependent variables, the regression includes lottery fixed effects. For class-level dependent variables, the regression is computed with propensity score weights. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables come from JUNAEB’s program implementation data sets.

4.2 Internal validity

Balancing checks

We test for baseline differences between the treatment and control groups by estimating Regression (1) with student- and teacher-level baseline measures as the dependent variables. First, Table A7 compares eligible students in the treatment and control groups on 29 baseline characteristics. Only two differences are significant at the 10% level: treatment group students are more disruptive as per enumerators' ratings, and they are more likely not to be nominated as a friend by any other student in the class. Only the first of those two differences is significant at the 5% level. Second, Table A10 compares ineligible students in the treatment and control groups on the same 29 baseline characteristics. Four differences are significant at the 10% level, one of which is also significant at the 5% level. Treatment group students have slightly worse social contact, attention and focus, activity level, and disruptiveness TOCA scores. Table A13 compares teachers in the treatment and control groups on 12 characteristics. Only one difference is significant at the 5% level. Finally, Table A14 compares six class-level characteristics in the treatment and control groups. Three differences are significant at the 10% level, one of which is significant at the 5% level. Treated classes are more disruptive than control ones according to teachers and enumerators, and have higher decibel levels.

Overall, we conduct 76 balancing checks in Tables A7, A10, A13, and A14. We find 10 significant differences between the treatment and control groups at the 10% level, four significant differences at the 5% level, and no significant difference at the 1% level.

Attrition

In this section, we document the percentage of students in our sample for which endline measures are not available, and the most common reasons for such attrition. We also show that the treatment and the control groups do not present differential levels of attrition, and that the characteristics of treatment and control group students for which endline measures are available are still balanced.

Table A5 considers attrition among eligible students. Column (1) shows the levels of attrition in the control group. Endline measures collected by the enumerators are missing for 25.2% of students. For 5.9% of them this is because they have left the class between baseline and endline, for instance because their parents have moved to a different neighborhood. For the most part, the remaining 19.3% are students who were absent on the day when the enumerators visited the class.⁷

⁷There are also a couple of classes that enumerators could not visit at endline, because the school principal did

The teacher’s endline disruptiveness rating is missing for 23.2% of students. Again, for some of them this is because they have left the class at endline. But for the majority of students, this is because their teachers refused to rate students’ disruptiveness, or only rated, say, the first half of the class and then stopped because they thought the task was too time-consuming. Column (2) of Table A5 shows tests of differential attrition between the treatment and control groups, conducted by estimating Regression (1) with measures of attrition as the dependent variables. Attrition does not seem differential: of the five measures we consider, only one is significantly different between the treatment and control groups at the 10% level.

Table A6 considers attrition among ineligible students. Columns (1) and (2) respectively show the levels of attrition in the control group, as well as tests for differential attrition between the treatment and control groups. The attrition levels in the control group are similar to those observed among eligible students. Here again, attrition is not differential: of the five measures we consider, only one is significantly different between the treatment and control groups at the 10% level.

Finally, we conduct balancing checks again, among the students whose endline measures are available. Table A8 (resp. Table A9) considers the same 29 baseline characteristics as in Table A7, and compares their mean in the treatment and control groups, among the eligible students for which enumerators’ endline measures (resp. the teacher’s endline disruptiveness rating) are (resp. is) available. As in Table A7, few differences are significant. Table A11 repeats the same exercise, among ineligible students for which enumerators’ endline measures are available. Again, few differences are significant. Finally, Table A12 compares ineligible students for which the teacher’s endline disruptiveness rating is available in the treatment and control groups. More differences are significant, but most become insignificant once p-values are adjusted for multiple testing. Overall, the post-attrition treatment and control group students whose outcomes are compared in Section 5 seem to have balanced baseline characteristics.

Turning to class-level attrition, while we have teachers’ and enumerators’ ratings of classes’ disruptiveness for more than 90% of classes in our sample, we have some differential attrition for teachers’ questionnaires: none is missing in the control group, while 8% are missing in the treatment group, and the difference is statistically significant. In Table A15, we conduct again the balancing checks on the baseline class-level measures in Table A14.⁸ For measures made by teachers, we

not want to sacrifice again a half day of instruction for the purpose of the study.

⁸Table A15 was not pre-specified in our PAP, because we had not anticipated the possibility of differential attrition for the class-level measures.

restrict the sample to classes for which all class-level endline teacher measures are available, while for measures made by enumerators we restrict the sample to classes for which all class-level endline enumerators measures are available. As in Table A14, three differences are significant at the 10% level, but none is significant at the 5% level.

4.3 Estimation methods

In this section, we discuss the methods we use to estimate the effect of the treatment. For our student-level outcomes, we estimate the following regression:

$$Y_{ijk} = \gamma_k + X'_{ijk}\theta_1 + \beta D_{jk} + u_{ijk}, \quad (2)$$

where Y_{ijk} is the outcome of student i in class j and lottery k , the γ_k s are lottery fixed effects, X_{ijk} denotes student-level baseline variables used as statistical controls, and D_{jk} is an indicator variable equal to 1 if class j in lottery k was assigned to the treatment group. $\hat{\beta}$ estimates the ITT effect of being assigned to the treatment on the outcome. As in Regression (1), we cluster the standard errors at the lottery level. To select the controls, we follow Belloni et al. [2014]. We run a Lasso regression of the outcome on all the student-level baseline variables in Table A7, and we pick the variables selected by the Lasso.⁹

For all the class-level outcomes, we estimate the following regression:

$$Y_{jk} = \alpha + Z'_{jk}\theta + \beta D_{jk} + u_{jk}, \quad (3)$$

where Y_{jk} is the outcome of class j in lottery k , Z_{jk} denotes class-level baseline variables used as statistical controls, and D_{jk} is the treatment indicator. The regression is weighted by propensity score weights, and as in Regression (1), we cluster the standard errors at the lottery level. To select the controls, we follow again Belloni et al. [2014], and we run a Lasso regression of the outcome on the class average of all the student-level baseline variables in Table A7, and all the class-level baseline variables in Tables A13 and A14, and we pick the variables selected by the Lasso.

To account for multiple testing, we follow the same approach as Finkelstein et al. [2010]. First, we group related outcomes into hypothesis. For instance, students' happiness, self-esteem, and self-control scores are grouped together into an "emotional stability" hypothesis. Then, for each outcome, we report both the unadjusted p-value of the estimated effect, and the adjusted p-value

⁹In a randomized experiment, the treatment is by construction uncorrelated with the controls, so it is not necessary to run a Lasso regression of the treatment on the controls.

controlling the FDR within the hypothesis the outcome belongs to. Each panel in Tables 3, 4, and 5 corresponds to a set of related outcomes grouped into an hypothesis. Finally, for each hypothesis we also report the effect of the treatment on a weighted average of the outcomes in that hypothesis, using the weights proposed in Anderson [2008]. We refer to the effect of the treatment on this weighted average as the standardized treatment effect.

5 Treatment Effects

5.1 Effects on eligible students

Table 3 below shows the effect of the SFL workshops on eligible students' outcomes.

Panel A shows that the SFL workshops do not have large effects on eligible students' emotional stability. The average school happiness score is 0.123σ higher in the treatment than in the control group, but this difference is not very significant (p-value=0.101), and becomes insignificant after adjusting for multiple testing. The average self-esteem score is 0.106σ lower in the treatment group, but this difference is insignificant even before adjusting for multiple testing (p-value=0.176). The average self-control score is very close in the treatment and control groups. Finally, the average standardized score is also very close in the treatment and control groups.

Panel B shows that SFL does not have a large effect on eligible students' disruptiveness. At endline, the average teachers' disruptiveness rating is 0.1σ higher in the treatment than in the control group. This difference is not statistically significant at conventional levels, but based on its estimated standard error, we can rule out at the 5% level that SFL reduces teachers' disruptiveness ratings by more than 0.1σ . This is around 1/5 of the treatment effect on students' disruptiveness found by Payton et al. [2008] in their meta-analysis of selected SEL programs. Enumerators' disruptiveness ratings also do not significantly differ in the treatment and control groups.

Panel C shows that SFL also does not have large effects on the academic outcomes of eligible students. For instance, students' Spanish and mathematics scores are very close in the two groups. We can reject at the 5% level that SFL increases eligible students' Spanish and mathematics scores by more than 0.086σ and 0.151σ , respectively. Again, these effects are much smaller than those found in the meta-analysis by Payton et al. [2008].

Finally, Panel D shows that SFL does not have large effects on eligible students' friendship ties. The proportion of students not nominated as a friend by any other student in the class is 2.8 percentage points lower in the treatment than in the control group, but this difference is insignificant.

Table 3: Treatment effect on eligible students

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: emotional stability						
School happiness score	-0.107	0.123	0.075	0.101	0.304	876
Self-control score	-0.184	-0.04	0.087	0.648	0.648	880
Self-esteem score	-0.17	-0.106	0.079	0.176	0.264	903
Standardized Treatment Effect	0.015	-0.002	0.08	0.977		915
Panel B: disruptiveness						
Disruptiveness, teacher	0.353	0.1	0.102	0.327	0.654	904
Disruptiveness, enumerator	0.157	0.02	0.083	0.805	0.805	948
Standardized Treatment Effect	-0.025	0.062	0.089	0.489		1110
Panel C: academic outcomes						
% school days missed	12.82	1.055	1.016	0.299	0.896	1236
Spanish test score	-0.308	-0.049	0.069	0.482	0.723	956
Math test score	-0.274	-0.006	0.08	0.945	0.945	956
Standardized Treatment Effect	0.011	-0.035	0.071	0.622		1238
Panel D: integration in the class network						
No friends in the class	0.27	-0.028	0.027	0.307	0.409	1147
% class friends with student	0.07	0.007	0.005	0.145	0.291	1147
Friends' average ability	-0.061	-0.011	0.077	0.883	0.883	829
Friends' average disruptiveness	0.177	0.132	0.087	0.131	0.525	787
Standardized Treatment Effect	-0.008	0.038	0.063	0.54		1148

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator, lottery fixed effects, and control variables for eligible students. The control variables are selected by a Lasso regression of the dependent variable on all potential controls, following Belloni et al. [2014]. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables, except for *% school days missed*, were collected by the authors at endline.

Overall, we do not find evidence of a positive effect of SFL on any of the dimensions we consider, and we can also rule out much smaller effects than those previously found for similar programs.

5.2 Effects on ineligible students

In this section, we explore whether the SFL workshops have spillover effects on ineligible students. Panel A of Table 4 below shows that these workshops do not generate strong spillover effects on the emotional stability of ineligible students. The average school happiness, self-control, and self-esteem scores are very close and do not significantly differ in the treatment and control groups.

Table 4: Treatment effect on ineligible students

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: emotional stability						
School happiness score	0.026	0.016	0.037	0.666	0.666	3360
Self-control score	0.097	-0.05	0.043	0.25	0.751	3404
Self-esteem score	0.084	-0.043	0.047	0.36	0.54	3446
Standardized Treatment Effect	0.027	-0.023	0.042	0.577		3476
Panel B: disruptiveness						
Disruptiveness, teacher	-0.212	0.208	0.106	0.05	0.101	3203
Disruptiveness, enumerator	-0.046	-0.003	0.046	0.954	0.954	3518
Standardized Treatment Effect	-0.051	0.063	0.072	0.384		4033
Panel C: academic outcomes						
% school days missed	13.089	0.382	0.634	0.547	0.82	4427
Spanish test score	0.128	-0.055	0.055	0.316	0.948	3517
Math test score	0.08	-0.013	0.056	0.821	0.821	3517
Standardized Treatment Effect	0.018	-0.019	0.044	0.66		4452
Panel D: integration in the class network						
No friends in the class	0.197	-0.035	0.013	0.008	0.033	4168
% class friends with student	0.087	0.004	0.003	0.156	0.312	4168
Friends' average ability	0.027	-0.011	0.077	0.884	0.884	3342
Friends' average disruptiveness	-0.11	0.051	0.053	0.338	0.45	3176
Standardized Treatment Effect	0.003	0.066	0.037	0.076		4171

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator, lottery fixed effects, and control variables for ineligible students. The control variables are selected by a Lasso regression of the dependent variable on potential controls, following Belloni et al. [2014]. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables, except for *% school days missed*, were collected by the authors at endline.

Panel B suggests that the SFL workshops may generate negative spillover effects on ineligible students’ disruptiveness. At endline, the average of teachers’ disruptiveness ratings is 0.208σ higher in the treatment than in the control group. This difference is significant (p-value=0.05), but becomes marginally insignificant after adjusting for multiple testing (adjusted p-value=0.101).

Then, Panel C shows that the SFL workshops do not have large spillover effects on ineligible students’ academic outcomes. Finally, Panel D shows that SFL improves the integration of ineligible students in the class network. The proportion of students not nominated as a friend by any other student in the class is 3.5 percentage points lower in the treatment than in the control group, a 17.8% reduction in the fraction of ineligible students who have no friends. This difference is significant (p-value=0.008), and it remains significant after accounting for multiple testing (adjusted p-value=0.033). Similarly, ineligible students are nominated as friends by 9.1% of their classmates in the treatment group, against 8.7% in the control group, but this difference is not significant. The treatment does not significantly alter the academic ability and disruptiveness of ineligible students’ friends. Finally, the average standardized score constructed from these four outcomes is significantly higher in the treatment than in the control group (p-value=0.076).

5.3 Effects on the classroom environment

Table 5: Treatment effect on classroom environment

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Disruptiveness, teacher	-0.187	0.232	0.137	0.091	0.226	160
Bullying in class, teacher	-0.038	0.105	0.153	0.492	0.492	160
Disruptiveness, enumerator	-0.186	0.389	0.148	0.009	0.043	167
Delay in class’s start (minutes)	9.938	1.204	1.046	0.25	0.312	160
Average decibels during class	0.022	0.681	0.487	0.162	0.27	169
Standardized Treatment Effect	-0.215	0.424	0.131	0.001		169

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and control variables, computed with propensity score weights. The control variables are selected by a Lasso regression of the dependent variable on all potential controls, following Belloni et al. [2014]. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables were collected by the authors at endline.

In this section, we study how the SFL workshops affect different measures of classrooms’ environment at endline. Table 5 above shows that SFL worsens teachers’ and enumerators’ disruptive-

ness ratings of the classes. Those ratings are based on teachers’ and enumerators’ agreement with statements like “There is noise and disorder in this class,” or “The teacher has to wait for a long time before students calm down and he/she can start teaching”. According to teachers, treated classes are 0.232σ more disruptive than control ones. This difference is statistically significant before adjusting for multiple testing (p-value=0.091), but it becomes insignificant after adjusting for it (adjusted p-value=0.226). According to enumerators, treated classes are 0.389σ more disruptive. This difference is statistically significant before and after adjusting for multiple testing (p-value=0.009, adjusted p-value=0.043). Enumerators do not know if the class they observe has been treated or not, contrary to teachers. The fact that they also find that treated classes are more disruptive suggests that teachers’ worse perception of the treatment-group classes is not a mere placebo effect. Table A14 in the Appendix shows that treated and control classes are imbalanced on these two measures at baseline, so we reestimate these two effects controlling for these two measures.¹⁰ The estimated treatment effects on teachers’ and enumerators’ ratings are now respectively equal to 0.247σ (p-value=0.084) and 0.282σ (p-value=0.066), so the treatment effects on these two measures do not seem due to imbalances already existing at baseline.

It may be surprising that the treatment significantly worsens enumerators’ ratings of classes’ overall disruptiveness, without affecting their ratings of eligible and ineligible students’ disruptiveness, as shown in Panel B of Tables 3 and 4. While the limited amount of time they spend in each classroom may be enough for them to observe that there is more disorder in the treated classes, it may not be sufficient for them to pinpoint the students responsible for that disorder.

Table 5 also shows that treated classes have higher levels of bullying, that their lectures start 1.2 more minutes after the scheduled time than in control classes, and that they have higher levels of decibels. Even though these results are not statistically significant, they go in the same direction as the results on the disruptiveness measures.

Finally, the average standardized score constructed from the five outcomes in Table 5 is 0.424σ higher in the treatment than in the control group. This difference is highly significant (p-value=0.001), and it remains highly significant even accounting for the fact that in Tables 3, 4, and 5 we estimate the effect of the treatment on nine standardized scores (adjusted p-value=0.009). Therefore, we can conclude that SFL significantly worsens the studying conditions in treated classes.

¹⁰In the estimation of the treatment effect on teachers’ ratings, the Lasso selects teachers’ baseline ratings as a control, but it does not select enumerators’ ratings. In the estimation of the treatment effect on enumerators’ ratings, the Lasso does not select any control.

5.4 Robustness checks

As a robustness check, we reestimate all the regressions in Tables 3, 4, and 5 without controls. The results of that exercise can be found in Tables B1, B2, and B3. Results with and without controls are pretty similar, except that the effects on ineligible students' friendships are no longer significant without controls. In our PAP, we had indicated that as a further robustness check, we would recompute all the unadjusted p-values in Tables 3, 4, and 5 using randomization inference. Doing so does not change our main findings so the results of that exercise are not reported here but are available upon request.

6 Interpretation and exploratory analysis

Table 3 shows that SFL does not have positive effects on eligible students' emotional stability, disruptiveness, and academic ability. This is at odds with an extensive literature, that has shown that selected SEL programs usually produce large positive effects on these dimensions. In a meta-analysis of 80 selected SEL interventions, Payton et al. [2008] find that they reduce conduct problems by 0.47σ , and respectively improve emotional stability and academic performance by 0.50 and 0.43σ . Based on our estimates, we can reject effects much smaller than those found in Payton et al. [2008].

There are several other meta-analyses of SEL interventions that have been peer-reviewed, unlike Payton et al. [2008], and that are more recent. However, they either focus on universal interventions delivered to the whole class rather than to a selected group of students (see [Durlak et al., 2011], [Sklad et al., 2012], [Wigelsworth et al., 2016], [Taylor et al., 2017], and [Corcoran et al., 2018]), or they include both universal and selected interventions but do not report effects separately for both types of interventions (see [Dymnicki et al., 2012]). To our knowledge, Payton et al. [2008] is the only meta-analysis reporting effects separately for selected SEL interventions comparable to SFL, which is why we focus on that meta-analysis. In any case, those six other meta-analyses also find pretty large effects, even though they are slightly lower than those in Payton et al. [2008]. The effects they find on conduct problems range from -0.14 to -0.47σ , with an average equal to -0.25σ . Similarly, effects on emotional stability range from 0.23 to 0.74σ , and the average is 0.55σ . Finally, effects on academic performance range from 0.26 to 0.53σ , and the average is 0.28σ . Therefore, we can still reject effects substantially smaller than the average effects in those meta-analyses. Overall, our results are at odds with a very substantial literature that has studied SEL programs.

To understand this discrepancy, in Table 6 below we compare SFL to the selected SEL inter-

ventions reviewed in Payton et al. [2008], to assess if SFL differs from those interventions in any striking way that could account for its lower effect. Many features of the interventions reviewed in Payton et al. [2008] are readily available from Table 7 therein. Other features that seemed important to us are not reported in the paper, so we reviewed a random sample of 25 of the meta-analysis’s papers, and manually collected those features. They appear in *italic* in Table 6.

6.1 SFL’s intensity is comparable to that of meta-analysis’s interventions

Panel A of Table 6 shows that SFL’s intensity is similar to that of the meta-analysis’s interventions. The median number of sessions across those interventions is slightly higher than SFL’s number of sessions (12 versus 10), but their sessions are typically shorter (50 versus 120 minutes). The number of students per workshop is comparable (a median of 6 in the meta-analysis, versus 7.2 on average in our sample). Their median duration is the same as SFL’s (10 weeks). 59% of those interventions only include sessions with students, while 41% also include a parental training, like SFL. Only seven of the papers we reviewed give the number of parental sessions, but among those the median number of sessions (14) is higher than in SFL (three parental sessions). Only three of the papers we reviewed mention parents’ attendance, but among those the median attendance (49%) is comparable to that in SFL (34%, see Table 2). Note also that Payton et al. [2008] do not mention that the presence and intensity of a parental training is correlated with larger program effects. In all those interventions, selected students are pulled-out of their class during the class day, as in the SFL intervention.¹¹

Panel B of Table 6 shows that SFL uses similar criteria as the programs reviewed by Payton et al. [2008] to determine which students are eligible. Like SFL, 69% of those interventions treat primary school students. 48% target students with conduct problems, 23% target students with emotional distress, and the remaining interventions target students with a combination of problems. 73% target low SES students, like SFL.

6.2 Our study design is comparable to that of metanalysis’s studies

Our study design is also comparable to that of the meta-analysis’s studies. Panel C of Table 6 shows that the treatment was randomly assigned in 80% of those studies. Many of the published studies appeared in high-impact-factor peer-reviewed journals (median impact factor=4.01).¹² Most of their outcome measures are teacher, enumerator, and student ratings, often made using validated

¹¹The three SFL teams we interviewed said that schools’ cooperation is usually very good, and that they do not have issues scheduling and delivering the sessions.

¹²85% of the 80 studies reviewed by Payton et al. [2008] were published in peer-reviewed journals.

Table 6: Comparing “Skills for Life” to the selected SEL interventions in Payton et al. [2008]

	Skills for Life	Payton et al. [2008]
Panel A: Intervention Intensity		
<i>Number of sessions</i>	10	12 (median)
<i>Sessions’ duration in minutes</i>	120	50 (median)
<i>Intervention duration in weeks</i>	10	10 (median)
<i>Number of students per workshop</i>	7.2	6 (median)
Parental training	Yes	41%
<i>Parental sessions</i>	3	14 (median)
<i>Parents’ attendance</i>	34%	49% (median)
Students pulled out of class	Yes	100%
Panel B: Targeting of eligible students		
Primary school students	Yes	69%
Students with conduct problems	Yes	48%
Students with emotional problems	Yes	23%
Students with conduct and emotional problems	Yes	29%
<i>Low SES students</i>	Yes	73%
Panel C: Study design		
Random assignment of treatment	Yes	80%
<i>Journal’s impact factor (for published studies)</i>	NA	4.01 (median)
Outcomes based on teacher ratings per study	3	0.6
Outcomes based on enumerator ratings per study	2	0.3
Outcomes based on student ratings per study	5	1.2875
Outcomes based on parent ratings per study	0	0.175
Outcomes based on school records per study	1	0.35
Uses validated psychometric scale as outcome	Yes	69%
<i>Weeks between end of intervention and endline</i>	3	1 (median)
Panel D: Location		
United States	No	85%
<i>High-income country</i>	No	100%
Panel E: Delivery Personnel, Monitoring of Delivery, and Intervention Scale		
Intervention delivered by:		
<i>Researchers (alone, or together with school staff)</i>	No	43%
<i>School staff trained and monitored by researchers</i>	No	22%
<i>Other personnel trained and monitored by researchers</i>	No	35%
<i>Frequency at which delivery is monitored:</i>	Never	Weekly (median)
<i>Number of treated students</i>	8,570	36 (median)

Notes: This table compares the “Skills for Life” intervention to those in the meta-analysis of Payton et al. [2008]. For the meta-analysis’s papers, the variables in italic were collected manually by the authors, by reviewing a random sample of 25 of the 80 articles reviewed by Payton et al. [2008]. The variables not in italic are directly available from Table 7 in Payton et al. [2008]. SFL’s number of treated students is for 2013.

psychometric scales, as in our study. We measured our outcomes three weeks after the end of the SFL intervention, while in the reviewed interventions, the median number of weeks between the end of the intervention and endline data collection is equal to one.

Another methodological concern is that our control group may have benefited from the treatment, as we have some schools that have both treated and control classes, and treated students may interact with students from control classes in their school. To assess if this is a serious concern, we estimate SFL’s effect in schools where only one class was included in our experiment. In this subsample, which still has 114 classes, we find that teachers’ ratings of eligible students’ disruptiveness is 0.2σ higher in the treatment than in the control group (p-value=0.12), and we can rule out at the 5% level that SFL reduces eligible students’ disruptiveness by more than 0.06σ . Results are similar when we consider other outcomes, such as students’ test scores. Overall, control-group contamination seems unlikely to account for SFL’s lack of effect.

6.3 Students receiving SFL may be harder to treat than those in the meta-analysis’s interventions.

Panel D of Table 6 shows that 85% of the interventions in Payton et al. [2008] take place in the US, and all take place in high-income countries. SFL takes place in Chile, and may then be faced with a harder-to-treat population than those interventions. For instance, recent epidemiological studies show that the prevalence rate of ADHD, a disorder correlated with conduct problems, is equal to 15.5% among primary school children in Chile (see [de la Barra et al., 2013]), against 6.8% in the US (see [Visser et al., 2014]), 3.5 to 5.6% in France (see [Lecendreux et al., 2011]) or 3% in Italy (see [Bianchini et al., 2013]). Similarly, surveys indicate that domestic violence, a cause of conduct disorder problems in children (see [Carrell and Hoekstra, 2010]), is more prevalent in Chile than in the US. 4.3% of Chilean women report having been physically assaulted by their partner over the previous year (see [Ministerio de Interior y de Seguridad Pública, 2017]), against 1.3% in the US (see [Tjaden and Thoennes, 2000]). Then, disruptive students may suffer from more severe problems in Chile than in high-income countries and may be harder to treat. This could explain why SFL produces lower effects than SEL programs in high-income countries.

To test this hypothesis, we start by assessing whether SFL’s effect is stronger for less disruptive students. Specifically, we look at the effect of SFL for eligible students with a TOCA score below the median. If anything, we find slightly negative effects: the program increases their teachers’ disruptiveness ratings by 0.18σ , but this effect is marginally significant (p-value=0.09).

Then, as primary school students with serious behavioral problems are more present in Chile than in high-income countries, each SFL workshop is more likely to comprise some very disruptive students than an SEL workshop in a high-income country, and the presence of those hard-to-treat students may lower the workshop’s effectiveness for every student, including the less disruptive ones. To investigate that possibility, we computed the 90th percentile¹³ of the average of the authority acceptance, attention and focus, activity levels, and overall disruptiveness TOCA scores among eligible students, and estimated SFL’s effects in the 79 classes that have at least one very disruptive eligible student above that threshold. Those classes have 123 very disruptive eligible students, 534 other eligible students, and 2,064 ineligible students. In Table 7 below, we estimate the effects separately for each group of students, focusing on disruptiveness ratings and test scores, and on the friendship nominations received by very disruptive eligible students. Unadjusted p-values and p-values controlling the False Discovery Rate (FDR) (see [Benjamini and Hochberg, 1995]) across all the tests in the table are presented.

First, Panel A of the table shows that the program does not have any statistically significant effect on the disruptiveness and test scores of very disruptive eligible students, but increases by 50% the percentage of their classmates who nominate them as friends (unadjusted p-value=0.042, adjusted p-value=0.102). Second, in Panel B we estimate SFL’s effects among the other eligible students. The program increases their teachers’ disruptiveness ratings by 0.496σ (unadjusted p-value=0.0006, adjusted p-value=0.0102), may reduce their Spanish scores by 0.201σ (unadjusted p-value=0.033, adjusted p-value=0.112), does not have a significant effect on their enumerators’ disruptiveness ratings and maths scores, and doubles the proportion that nominate at least one very disruptive student as a friend (unadjusted p-value=0.012, adjusted p-value=0.051). Very disruptive eligible students may then have a negative influence on other eligible students, which could explain the negative effects the program has on them. Third, in Panel C we estimate that the program increases teachers’ and enumerators’ disruptiveness ratings of ineligible students, respectively by 0.477σ (unadjusted p-value=0.008, adjusted p-value=0.045) and 0.137σ (unadjusted p-value=0.083, adjusted p-value=0.176). On the other hand, the program does not have a significant effect on the test scores of those students and on the proportion of them who nominate a very disruptive student as a friend. The mechanism whereby the program makes ineligible students more disruptive may be a contagion effect: eligible students become more disruptive, and ineligible students imitate them.

¹³The choice of the 90th percentile was guided by the fact that $0.9^7 = 48\%$, so assuming that students’ disruptiveness levels are independent within a class and that all classes have 7 eligible students, 52% of classes should have at least one student above that percentile. In practice, the proportion of classes that have at least one eligible student above that percentile is slightly lower (46%), but still close to 50%.

Finally, in Panel D we estimate that the program increases teachers' and enumerators' overall disruptiveness ratings of the classes, respectively by 0.669σ (unadjusted p-value=0.005, adjusted p-value=0.043) and 0.516σ (unadjusted p-value=0.035, adjusted p-value=0.099). The regressions in the table are estimated with the controls selected by the Lasso. Treatment effects are similar when those controls are dropped (see Appendix Table C1), and when the few covariates that are unbalanced at baseline in the relevant subsample are added as controls (see Appendix Table C2).

Classes with at least one very disruptive eligible student have slightly more eligible students than classes that do not have any (8.3 versus 6.2). This difference is not very large, but we still checked if we also find negative effects of the program in the subsample of classes that have more eligible students than the median. The answer is negative, so it does not seem that the negative effects we find in classes with at least one very disruptive eligible student are mediated by the slightly higher number of eligible students in those classes.

Overall, we find suggestive evidence that SFL's effectiveness is hampered by the presence of very disruptive students, who may be less present in the other contexts where SEL programs have been shown to work. We still do not find statistically significant effects of SFL in the 93 classes that do not have any very disruptive student. This may be because the effects we can reject in this subsample are too large, though we can for instance still reject at the 5% level an effect larger than 0.13σ on Spanish test scores. Another potential explanation is that even those classes may still have some students that are more disruptive than the typical students benefiting from selected SEL programs in the US.

Table 7: Treatment effect in classes with at least one very disruptive students

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: Very disruptive eligible students						
Disruptiveness, teacher	0.985	-0.175	0.330	0.600	0.850	86
Disruptiveness, enumerator	0.286	0.613	0.439	0.162	0.306	85
Spanish test score	-0.460	0.047	0.304	0.878	1	88
Math test score	-0.230	0.063	0.512	0.902	1	88
% class friends with student	0.051	0.025	0.012	0.042	0.102	109
Panel B: Not very disruptive eligible students						
Disruptiveness, teacher	0.294	0.496	0.145	0.001	0.010	391
Disruptiveness, enumerator	0.162	0.118	0.124	0.339	0.576	393
Spanish test score	-0.349	-0.201	0.097	0.033	0.112	397
Math test score	-0.349	-0.008	0.181	0.965	0.965	397
Friends with ≥ 1 very dis.	0.065	0.075	0.030	0.012	0.051	397
Panel C: Ineligible students						
Disruptiveness, teacher	-0.205	0.477	0.181	0.008	0.045	1517
Disruptiveness, enumerator	-0.093	0.137	0.079	0.083	0.176	1576
Spanish test score	0.035	0.012	0.122	0.924	0.982	1579
Math test score	0.115	0.053	0.151	0.725	0.948	1579
Friends with ≥ 1 very dis.	0.067	0.015	0.028	0.584	0.903	1577
Panel D: Class-level outcomes						
Disruptiveness, teacher	-0.250	0.669	0.236	0.005	0.043	72
Disruptiveness, enumerator	-0.250	0.516	0.245	0.035	0.099	76

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and control variables. The control variables are selected by a Lasso regression of the dependent variable on potential controls, following Belloni et al. [2014]. To account for the fact the randomization is stratified, the regressions in Panels A, B, and C have lottery fixed effects, while in the regressions in Panel D we use propensity score reweighting. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient. Finally, Column (5) reports the number of observations used in the regression. All the dependent variables were collected by the authors at endline.

6.4 SFL’s delivery is less monitored than the meta-analysis interventions’.

Panel E of Table 6 shows that SFL strikingly differs from the meta-analysis’s programs in terms of delivery. All of the meta-analysis’s interventions are demonstration programs, mounted by researchers for research purposes. 43% of the interventions are entirely or partly delivered by the researchers, 22% are delivered by school staff trained and supervised by the researchers, and 35% are delivered by other personnel (most often psychologists) hired, trained, and supervised by the

researchers. 69% of the studies where the intervention was not entirely delivered by the researchers mention the frequency at which the researchers monitored the delivery personnel, for instance by attending sessions, or by reviewing video- or audio-recorded sessions. The median is a weekly monitoring. Researchers' involvement is very high in the studies reviewed by Payton et al. [2008], but another meta-analysis of universal SEL interventions suggests that they can produce large effects without researchers' involvement. Wigelsworth et al. [2016] review 25 interventions implemented without researchers' involvement and find large effects: -0.15σ for conduct problems, $+0.47\sigma$ for emotional stability, and $+0.22\sigma$ for academic performance. However, looking at a random sample of 10 of those 25 studies, it appears that in 6 of the 8 studies where monitoring was discussed, monitoring was frequent and intensive, and was often conducted by an NGO promoting the program. Overall, in the majority of the SEL interventions considered in those meta-analysis, delivery is monitored frequently by a third party.

JUNAEB provides SFL implementers with a detailed manual describing the content of each of the workshop's session, and the municipal teams we interviewed said they follow this manual. SFL employees also attend "good practices" meetings every six months, during which they share with other teams what seems to work in their sessions. However, JUNAEB does not systematically and frequently monitor each team's delivery. Of the three teams we interviewed, only one had a workshop observed over the last two years.¹⁴ Then, SFL's lack of effect may be due to the lack of a frequent and intensive third-party monitoring of the workshops, unlike what is happening in the studies reviewed by Payton et al. [2008] and Wigelsworth et al. [2016]. Without sufficient monitoring, teams may not implement the program with high-enough fidelity. Unfortunately, beyond the striking difference between SFL and the reviewed interventions on that dimension, we cannot further support that conjecture by testing whether the treatment effect is larger for teams that are monitored more often or that implement SFL with higher fidelity. We do not observe the frequency at which each municipal team in our study is monitored by JUNAEB, and in any case our discussions with the teams and JUNAEB officials suggest monitoring is weak in every town. Similarly, we do not have a measure of implementers' fidelity and we do not observe implementers' number of years of experience into the program, which could be a proxy for fidelity.

¹⁴SFL employees also do not have monetary or non-monetary incentives tied to the quality of their workshops.

7 Conclusion

We explore the effects of “Skills for life” (SFL), a nationwide school-based SEL program for disruptive second graders in Chile. Eligibility to the program is based on first-grade teachers’ ratings of students’ disruptiveness, and SFL workshops consist in 10 two-hours sessions during which psychologists help students recognize and express their emotions, and teach them techniques to improve their behavior. We randomly assigned 172 classes to either receive SFL in the first or in the second semester of the 2015 school year, and we measured outcomes between the two semesters. Eligible students in treated classes see no improvement in their emotional stability, disruptiveness, and test scores. This is at odds with a large literature that has found large effects of SEL programs (see [Payton et al., 2008], [Durlak et al., 2011], [Dymnicki et al., 2012], [Sklad et al., 2012], [Wigelsworth et al., 2016], [Taylor et al., 2017], and [Corcoran et al., 2018] for recent meta-analyses).

To understand this discrepancy, we investigate the differences between SFL and the programs studied in the literature. First, we find that SFL is not less intensive than those other programs. Second, its population may be harder to treat: all the programs studied in the literature take place in high-income countries, where the prevalence of ADHD, a disorder correlated with conduct problems, is much lower than in Chile. Accordingly, each SFL workshop is more likely to comprise one or two very disruptive students than an SEL workshop in a high-income country, and the presence of those hard-to-treat students may lower the workshop’s effectiveness. We actually find evidence that SFL may increase students’ disruptiveness in classes that have at least one very disruptive eligible student. The mechanism seems to be that SFL increases the friendships between very disruptive and other eligible students. Then, very disruptive students may have a negative influence on those other eligible students. To remediate this, SFL could exclude very disruptive students from its workshops, and offer them another type of treatment, for instance one-on-one sessions with a psychologist. Third, the literature has only considered small-scale programs mounted by researchers or NGOs, and either delivered by the researchers or NGO personnel, or by personnel closely monitored by them. On the other hand, SFL is a governmental program, and the government does not monitor the workshops’ content and quality. Most of the interventions reviewed in the literature are implemented at a very small scale, in a handful of schools: the median number of treated students in the interventions reviewed by Payton et al. [2008] is equal to 36. On the other hand, SFL treats around 8,500 students per year, in thousands of schools. Monitoring SFL

as intensively as the small-scale interventions in Payton et al. [2008] would probably be costly, but our results suggests this may be worth trying.

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Appendix A Tables

Table A1: Baseline - endline correlations in the control group

	Correlation (1)	P-value (2)	N (3)
Panel A: student-level measures			
School happiness score	0.221	0.000	1735
Self-control score	0.141	0.000	1816
Self-esteem score	0.134	0.000	1841
Disruptiveness, teacher	0.419	0.000	1782
Disruptiveness, enumerator	0.126	0.000	1871
% school days missed	0.033	0.084	2751
Spanish test score	0.525	0.000	1897
Math test score	0.508	0.000	1897
% class friends with student	0.324	0.000	2245
Friends' average ability	0.408	0.000	1644
Friends' average disruptiveness	0.349	0.000	1502
No friends in the class	0.099	0.000	2245
Panel B: class-level measures			
Disruptiveness, teacher	0.5	0.000	78
Bullying in class, teacher	0.392	0.000	76
Disruptiveness, enumerator	0.254	0.024	79
Average decibels during class	0.152	0.18	79
Delay in class's start (minutes)	0.031	0.788	79

Notes: This table reports the correlation, in control classes, of several covariates between baseline and endline. Column (1) reports the baseline - endline correlation of the covariates. Column (2) reports the p-value of the significance of the correlation. Column (3) reports the number of observations used to compute the correlation.

Table A2: Correlations between baseline disruptiveness measures

	Correlation (1)	P-value (2)	N (3)
Panel A: student-level measures			
Enumerator 1 - enumerator 2	0.504	0.000	4075
Teacher - enumerator	0.293	0.000	4035
Teacher dis. - avg. test score	-0.277	0.000	4139
Enumerator dis. - avg. test score	-0.17	0.000	4594
Panel B: class-level measures			
Enumerator 1 - Enumerator 2	0.618	0.000	157
Enumerator - Teacher	0.337	0.000	159
Enumerator - decibels	0.2	0.011	163
Teacher - decibels	-0.018	0.82	157

Notes: This table reports the correlation, in control classes, between several baseline measures of disruption. Column (1) reports the correlation between the measures. Column (2) reports the p-value of the significance of the correlation. Column (3) reports the number of observations used to compute the correlation.

Table A3: Characteristics of takers and non-takers

	Non-takers (1)	Takers (2)	P-value (3)	N (4)
Panel A: demographic characteristics				
Male	0.667	0.567	0.05	655
Teen mother	0.415	0.368	0.43	525
Student lives with father	0.515	0.551	0.577	478
\leq p20 social security score	0.842	0.741	0.016	596
\leq p5 social security score	0.463	0.441	0.693	596
Mother's education	8.448	8.327	0.798	576
Father's education	8.014	8.198	0.727	485
Panel B: baseline measures				
School happiness score	0.08	-0.034	0.41	477
Self-control score	-0.27	-0.172	0.493	511
Self-esteem score	-0.233	-0.176	0.708	513
Overall disruptiveness TOCA	1.128	0.81	0.011	645
Disruptiveness, enumerator	0.7	0.397	0.051	517
Spanish test score	-0.496	-0.326	0.22	548
Math test score	-0.489	-0.248	0.085	548
% class friends with student	0.069	0.079	0.168	539
Friends' average disruptiveness	0.324	0.241	0.604	422

Notes: This table reports descriptive statistics for eligible students, comparing those who attended and did not attend the workshops. Column (1) reports the mean of the outcome variable for eligible students who did not attend any session. Column (2) reports the mean of the variable for eligible students who attended at least one session. Column (3) reports the p-value of a test that the two means are equal. Column (4) reports the number of observations used in the comparison.

Table A4: Characteristics of teachers

	Mean (1)	N (2)
Female	0.963	160
Age	42.78	159
University degree	0.863	160
Years of experience	16.547	161
Years of experience, school	8.568	162

Notes: This table reports descriptive statistics for teachers in the sample. Column (1) reports the mean of the variables and Column (2) reports the number of observations used to compute that mean.

Table A5: Test of differential attrition for eligible students

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Eligible students per class at endline	6.651	0.473	0.386	0.22	0.55	169
Join class btw baseline and endline	0.023	0.004	0.008	0.649	0.649	1229
In class at baseline and endline	0.941	0.024	0.014	0.078	0.389	1178
With all enumerators' measures	0.748	-0.035	0.03	0.247	0.308	1238
With teacher's disruption measure	0.768	-0.084	0.071	0.235	0.392	1238

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator. For student-level dependent variables, the regression includes lottery fixed effects. For class-level dependent variables, the regression is computed with propensity score weights. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables were collected by the authors at endline.

Table A6: Test of differential attrition for ineligible students

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Ineligible students per class at endline	25.518	-1.009	0.853	0.237	0.592	169
Join class btw baseline and endline	0.045	-0.005	0.008	0.553	0.691	4433
In class at baseline and endline	0.962	-0.001	0.007	0.842	0.842	4159
With all enumerators' measures	0.783	-0.048	0.027	0.074	0.371	4466
With teacher's disruption measure	0.753	-0.059	0.067	0.383	0.638	4466

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator. For student-level dependent variables, the regression includes lottery fixed effects. For class-level dependent variables, the regression is computed with propensity score weights. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables were collected by the authors at endline.

Table A7: Balancing tests of eligible students' baseline characteristics

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: demographic characteristics						
Male	0.581	-0.004	0.046	0.937	0.97	1238
Teen mother	0.343	0.018	0.031	0.549	0.885	991
Student lives with father	0.563	-0.012	0.034	0.726	1	899
Social security score	5564.943	137.239	173.203	0.428	0.828	1124
Payment rate in health services	2.879	0.327	0.361	0.365	0.963	1122
Mother's education	8.813	-0.292	0.32	0.362	1	1080
Father's education	8.743	-0.565	0.38	0.137	0.995	913
Panel B: TOCA and PSC scores						
Authority Acceptance TOCA	1.027	-0.084	0.063	0.181	0.751	1223
Social Contact TOCA	0.842	-0.025	0.072	0.723	1	1223
Motiv. for Schooling TOCA	0.842	-0.036	0.06	0.543	0.985	1223
Emotional Maturity TOCA	0.563	-0.12	0.076	0.117	1	1223
Attention and Focus TOCA	0.834	-0.054	0.063	0.391	0.873	1223
Activity Level TOCA	0.831	-0.054	0.064	0.404	0.837	1223
Academic ability TOCA	0.667	-0.016	0.071	0.82	0.951	1222
Overall disruptiveness TOCA	0.891	-0.046	0.076	0.548	0.935	1220
PSC	0.477	-0.011	0.08	0.889	0.955	903
Panel C: baseline measures						
School happiness score	-0.107	0.082	0.083	0.323	1	929
Self-control score	-0.148	-0.057	0.063	0.371	0.897	986
Self-esteem score	-0.107	-0.105	0.076	0.168	0.811	991
Disruptiveness, teacher	0.396	0.087	0.276	0.753	0.993	253
Disruptiveness, enumerator	0.242	0.204	0.085	0.017	0.484	1007
Spanish test score	-0.321	-0.021	0.086	0.806	0.973	1036
Math test score	-0.301	0.021	0.099	0.829	0.924	1036
% class friends with student	0.075	0.002	0.006	0.769	0.97	1030
Friends' average ability	-0.09	-0.002	0.114	0.988	0.988	863
Friends' average disruptiveness	0.122	0.099	0.103	0.333	1	822
No friends in the class	0.128	0.047	0.026	0.065	0.938	1030
Distance to teacher's desk	4.361	-0.079	0.18	0.66	1	863
% school days missed, March	36.971	-4.809	3.421	0.16	0.927	1236

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and lottery fixed effects for eligible students. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression.

Table A8: Balancing tests of eligible students' baseline characteristics, for those with all enumerators' endline measures.

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: demographic characteristics						
Male	0.56	0.016	0.054	0.767	1	906
Teen mother	0.324	0.081	0.04	0.044	0.632	731
Student lives with father	0.58	-0.051	0.038	0.183	0.883	665
Social security score	5640.612	-62.531	227.803	0.784	1	819
Payment rate in health services	3.005	0.122	0.472	0.795	1	824
Mother's education	8.836	-0.218	0.404	0.589	1	794
Father's education	8.768	-0.197	0.396	0.619	1	667
Panel B: TOCA and PSC scores						
Authority Acceptance TOCA	1	-0.038	0.063	0.548	1	894
Social Contact TOCA	0.785	0.008	0.077	0.919	0.987	894
Motiv. for Schooling TOCA	0.809	-0.009	0.065	0.893	1	894
Emotional Maturity TOCA	0.591	-0.128	0.083	0.123	0.895	894
Attention and Focus TOCA	0.798	0.013	0.064	0.845	1	894
Activity Level TOCA	0.821	-0.026	0.07	0.713	1	894
Academic ability TOCA	0.626	-0.014	0.079	0.859	1	894
Overall disruptiveness TOCA	0.801	0.034	0.092	0.712	1	893
PSC	0.441	-0.005	0.089	0.957	0.991	669
Panel C: baseline measures						
School happiness score	-0.077	0.069	0.091	0.445	1	700
Self-control score	-0.136	-0.018	0.079	0.824	1	745
Self-esteem score	-0.13	-0.043	0.093	0.643	1	744
Disruptiveness, teacher	0.341	0.061	0.215	0.776	1	192
Disruptiveness, enumerator	0.201	0.203	0.095	0.033	0.957	742
Spanish test score	-0.264	-0.01	0.084	0.908	1	769
Math test score	-0.22	0.037	0.11	0.736	1	769
% class friends with student	0.077	0.006	0.006	0.353	1	765
Friends' average ability	-0.071	0.000	0.126	0.997	0.997	656
Friends' average disruptiveness	0.094	0.162	0.118	0.17	0.987	623
No friends in the class	0.111	0.048	0.026	0.068	0.657	765
Distance to teacher's desk	4.377	-0.203	0.225	0.366	1	630
% school days missed, March	37.887	-4.312	3.658	0.238	0.988	904

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and lottery fixed effects for eligible students with all enumerators' endline measures. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression.

Table A9: Balancing tests of eligible students' baseline characteristics, for those with teacher's endline disruptiveness measure.

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: demographic characteristics						
Male	0.574	-0.01	0.053	0.848	0.984	901
Teen mother	0.337	0.033	0.038	0.394	0.952	724
Student lives with father	0.564	-0.006	0.045	0.89	0.922	659
Social security score	5533.873	205.674	236.641	0.385	1	814
Payment rate in health services	3.144	-0.045	0.506	0.929	0.929	816
Mother's education	8.897	-0.594	0.415	0.152	0.883	798
Father's education	8.771	-0.483	0.511	0.345	1	673
Panel B: TOCA and PSC scores						
Authority Acceptance TOCA	0.983	-0.12	0.08	0.136	0.983	889
Social Contact TOCA	0.829	0.041	0.096	0.666	1	889
Motiv. for Schooling TOCA	0.852	-0.018	0.081	0.821	0.992	889
Emotional Maturity TOCA	0.597	-0.123	0.1	0.219	1	889
Attention and Focus TOCA	0.842	-0.046	0.082	0.572	0.922	889
Activity Level TOCA	0.821	-0.124	0.081	0.124	1	889
Academic ability TOCA	0.676	-0.052	0.091	0.563	0.961	888
Overall disruptiveness TOCA	0.877	-0.069	0.099	0.482	0.999	887
PSC	0.434	-0.017	0.103	0.869	0.933	662
Panel C: baseline measures						
School happiness score	-0.064	-0.064	0.096	0.503	0.912	680
Self-control score	-0.128	-0.165	0.085	0.053	0.762	718
Self-esteem score	-0.078	-0.106	0.088	0.23	0.952	720
Disruptiveness, teacher	0.275	0.057	0.245	0.815	1	190
Disruptiveness, enumerator	0.193	0.107	0.105	0.31	1	743
Spanish test score	-0.34	0.03	0.088	0.736	1	758
Math test score	-0.28	0.036	0.133	0.786	1	758
% class friends with student	0.075	0.006	0.008	0.451	1	751
Friends' average ability	-0.138	0.129	0.143	0.367	1	635
Friends' average disruptiveness	0.129	0.026	0.14	0.853	0.951	611
No friends in the class	0.102	0.088	0.035	0.011	0.31	751
Distance to teacher's desk	4.441	0.061	0.178	0.732	1	643
% school days missed, March	37.204	-2.795	4.143	0.5	0.966	899

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and lottery fixed effects for eligible students with teacher's endline disruptiveness measure. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression.

Table A10: Balancing tests of ineligible students' baseline characteristics.

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: demographic characteristics						
Male	0.486	0.026	0.027	0.327	0.678	4466
Teen mother	0.328	0.016	0.02	0.434	0.662	3449
Student lives with father	0.639	-0.012	0.017	0.501	0.727	2866
Social security score	5965.036	-108.938	107.006	0.309	0.746	3944
Payment rate in health services	4.132	-0.019	0.313	0.951	0.951	3927
Mother's education	9.239	-0.19	0.2	0.341	0.582	3647
Father's education	9.181	-0.017	0.177	0.925	0.958	3204
Panel B: TOCA and PSC scores						
Authority Acceptance TOCA	-0.356	0.059	0.054	0.278	1	3654
Social Contact TOCA	-0.346	0.14	0.055	0.01	0.297	3654
Motiv. for Schooling TOCA	-0.312	0.071	0.047	0.132	0.765	3654
Emotional Maturity TOCA	-0.171	0.024	0.092	0.795	0.922	3654
Attention and Focus TOCA	-0.32	0.092	0.053	0.086	0.624	3654
Activity Level TOCA	-0.33	0.124	0.066	0.059	0.86	3645
Academic ability TOCA	-0.244	0.043	0.041	0.292	0.847	3633
Overall disruptiveness TOCA	-0.335	0.075	0.041	0.068	0.66	3630
PSC	-0.171	0.043	0.044	0.333	0.603	2882
Panel C: baseline measures						
School happiness score	0.039	-0.015	0.039	0.697	0.879	3502
Self-control score	0.05	-0.005	0.045	0.917	0.985	3608
Self-esteem score	0.066	-0.051	0.043	0.234	0.971	3619
Disruptiveness, teacher	-0.132	0.052	0.181	0.772	0.933	804
Disruptiveness, enumerator	-0.067	0.078	0.06	0.193	0.933	3638
Spanish test score	0.139	-0.065	0.076	0.393	0.632	3722
Math test score	0.083	0.033	0.079	0.676	0.891	3722
% class friends with student	0.09	-0.003	0.005	0.523	0.722	3691
Friends' average ability	0.055	0.017	0.099	0.86	0.959	3260
Friends' average disruptiveness	-0.094	0.075	0.073	0.305	0.804	3109
No friends in the class	0.097	0.02	0.02	0.328	0.635	3691
Distance to teacher's desk	4.519	0.168	0.158	0.286	0.923	3129
% school days missed, March	38.922	-2.992	2.969	0.314	0.699	4427

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and lottery fixed effects for ineligible students. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression.

Table A11: Balancing tests of ineligible students' baseline characteristics, for those with all enumerators' endline measures.

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: demographic characteristics						
Male	0.473	0.038	0.027	0.154	0.64	3376
Teen mother	0.322	0.015	0.021	0.481	0.734	2646
Student lives with father	0.647	-0.008	0.021	0.702	0.783	2203
Social security score	5982.408	-99.568	119.473	0.405	0.903	2989
Payment rate in health services	4.305	-0.181	0.376	0.63	0.795	2974
Mother's education	9.239	-0.184	0.223	0.409	0.847	2788
Father's education	9.189	0.022	0.19	0.908	0.941	2454
Panel B: TOCA and PSC scores						
Authority Acceptance TOCA	-0.365	0.054	0.05	0.282	0.745	2768
Social Contact TOCA	-0.39	0.173	0.061	0.005	0.138	2768
Motiv. for Schooling TOCA	-0.351	0.074	0.05	0.137	0.661	2768
Emotional Maturity TOCA	-0.182	0.079	0.103	0.44	0.751	2768
Attention and Focus TOCA	-0.346	0.095	0.052	0.069	0.501	2768
Activity Level TOCA	-0.331	0.164	0.061	0.007	0.108	2762
Academic ability TOCA	-0.28	0.05	0.045	0.264	0.766	2759
Overall disruptiveness TOCA	-0.363	0.075	0.038	0.045	0.436	2756
PSC	-0.195	0.047	0.058	0.417	0.807	2210
Panel C: baseline measures						
School happiness score	0.045	-0.018	0.045	0.688	0.798	2715
Self-control score	0.07	-0.021	0.05	0.673	0.813	2789
Self-esteem score	0.102	-0.081	0.051	0.112	0.651	2797
Disruptiveness, teacher	-0.208	0.101	0.167	0.545	0.752	641
Disruptiveness, enumerator	-0.06	0.048	0.061	0.434	0.787	2805
Spanish test score	0.171	-0.067	0.071	0.347	0.838	2870
Math test score	0.106	0.042	0.08	0.598	0.789	2870
% class friends with student	0.09	0.000	0.006	0.95	0.95	2852
Friends' average ability	0.073	0.012	0.099	0.904	0.971	2524
Friends' average disruptiveness	-0.095	0.101	0.075	0.176	0.636	2402
No friends in the class	0.098	0.014	0.022	0.515	0.746	2852
Distance to teacher's desk	4.522	0.124	0.163	0.446	0.718	2416
% school days missed, March	38.416	-3.897	3.252	0.231	0.744	3353

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and lottery fixed effects for ineligible students with all enumerators' endline measures. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression.

Table A12: Balancing tests of ineligible students' baseline characteristics, for those with teacher's endline disruptiveness measure.

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: demographic characteristics						
Male	0.486	0.061	0.03	0.043	0.248	3202
Teen mother	0.319	0.04	0.025	0.118	0.381	2490
Student lives with father	0.641	0.012	0.023	0.61	0.804	2071
Social security score	5966.787	18.269	149.837	0.903	1	2838
Payment rate in health services	4.271	-0.156	0.42	0.71	0.823	2826
Mother's education	9.281	-0.293	0.281	0.296	0.506	2637
Father's education	9.276	-0.151	0.272	0.579	0.8	2310
Panel B: TOCA and PSC scores						
Authority Acceptance TOCA	-0.347	0.056	0.067	0.405	0.652	2645
Social Contact TOCA	-0.378	0.24	0.075	0.001	0.041	2645
Motiv. for Schooling TOCA	-0.323	0.122	0.055	0.028	0.267	2645
Emotional Maturity TOCA	-0.136	0.012	0.116	0.915	0.948	2645
Attention and Focus TOCA	-0.329	0.121	0.055	0.027	0.393	2645
Activity Level TOCA	-0.308	0.082	0.074	0.27	0.56	2637
Academic ability TOCA	-0.245	0.06	0.049	0.222	0.536	2632
Overall disruptiveness TOCA	-0.328	0.104	0.048	0.032	0.229	2630
PSC	-0.172	0.075	0.06	0.212	0.558	2084
Panel C: baseline measures						
School happiness score	0.047	-0.061	0.05	0.227	0.506	2531
Self-control score	0.106	-0.117	0.058	0.046	0.22	2592
Self-esteem score	0.09	-0.107	0.063	0.089	0.367	2604
Disruptiveness, teacher	-0.268	0.285	0.172	0.097	0.353	634
Disruptiveness, enumerator	-0.095	0.083	0.065	0.201	0.582	2638
Spanish test score	0.118	0.009	0.078	0.906	0.973	2689
Math test score	0.094	0.059	0.101	0.56	0.813	2689
% class friends with student	0.091	0.000	0.005	0.937	0.937	2659
Friends' average ability	0.045	0.058	0.123	0.635	0.767	2366
Friends' average disruptiveness	-0.073	0.096	0.091	0.289	0.523	2259
No friends in the class	0.088	0.023	0.021	0.277	0.536	2659
Distance to teacher's desk	4.565	0.158	0.226	0.483	0.738	2355
% school days missed, March	39.314	-1.844	3.663	0.615	0.775	3178

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and lottery fixed effects for ineligible students with teacher's endline disruptiveness measure. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression.

Table A13: Balancing tests of teachers' baseline characteristics

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: demographic characteristics						
Age	43.013	-0.256	1.763	0.885	0.965	159
University degree	0.872	-0.019	0.06	0.748	1	160
Years of experience	16.367	0.508	2.108	0.809	1	161
Years of experience in the school	8.139	0.729	1.331	0.584	1	162
Absenteeism	0.646	-0.101	0.547	0.853	1	162
Panel B: motivation and taste for their job						
Taste for her job	0.007	0.031	0.144	0.827	1	161
Confident to improve students' life	0.076	-0.146	0.172	0.395	1	161
Effort to prepare lectures	0.497	0.023	0.042	0.588	1	143
Diverse methods used in class	-0.005	0.016	0.161	0.919	0.919	161
Panel C: mental health						
Stress score	0.073	-0.138	0.156	0.377	1	160
Happiness score	0.148	-0.317	0.15	0.034	0.41	161
Control on life score	0.054	-0.115	0.151	0.447	1	158

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator for teachers. The regression is estimated with propensity score weights. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables were collected by the authors at baseline.

Table A14: Balancing tests of classes' baseline characteristics

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Academic level of the class, teacher	0.059	-0.086	0.14	0.538	0.538	162
Disruptiveness, teacher	-0.143	0.286	0.16	0.074	0.148	161
Bullying in class, teacher	0.033	-0.094	0.147	0.519	0.623	160
Disruptiveness, enumerator	-0.131	0.275	0.153	0.072	0.217	168
Delay in class's start (minutes)	8.802	1.122	1.253	0.37	0.555	166
Average decibels during class	0.053	1.796	0.745	0.016	0.095	165

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator. The regression is estimated with propensity score weights. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables were collected by the authors at baseline.

Table A15: Balancing tests of classes' baseline characteristics, for classes with all teacher's or enumerators' endline measures.

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: classes with all teacher's measures						
Academic level of the class, teacher	0.052	-0.095	0.143	0.509	0.611	150
Disruptiveness, teacher	-0.145	0.326	0.17	0.055	0.332	149
Bullying in class, teacher	0.036	-0.099	0.158	0.532	0.532	148
Panel B: classes with all enumerators' measures						
Disruptiveness, enumerator	-0.136	0.277	0.152	0.068	0.205	155
Average decibels during class	-0.108	1.391	0.815	0.088	0.176	153
Delay in class's start (minutes)	8.885	1.424	1.412	0.313	0.469	153

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator for classes with all teacher's or enumerators' measures. The regression is estimated with propensity score weights. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables were collected by the authors at baseline.

Appendix B Results without controls

Table B1: Treatment effect on eligible students

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: emotional stability						
School happiness score	-0.107	0.136	0.082	0.097	0.292	876
Self-control score	-0.184	-0.04	0.09	0.654	0.654	880
Self-esteem score	-0.17	-0.107	0.081	0.183	0.275	903
Standardized Treatment Effect	0.015	-0.002	0.08	0.977		915
Panel B: disruptiveness						
Disruptiveness, teacher	0.353	0.057	0.099	0.562	1	904
Disruptiveness, enumerator	0.157	0.017	0.083	0.842	0.842	948
Standardized Treatment Effect	-0.025	0.041	0.088	0.645		1110
Panel C: academic outcomes						
% school days missed	12.82	1.055	1.016	0.299	0.896	1236
Spanish test score	-0.308	-0.044	0.082	0.59	0.886	956
Math test score	-0.274	-0.006	0.081	0.946	0.946	956
Standardized Treatment Effect	0.011	-0.049	0.083	0.555		1238
Panel D: integration in the class network						
% class friends with student	0.07	0.008	0.005	0.118	0.472	1147
Friends' average ability	-0.061	-0.022	0.096	0.816	0.816	829
Friends' average disruptiveness	0.177	0.146	0.096	0.13	0.259	787
No friends in the class	0.27	-0.025	0.027	0.348	0.464	1147
Standardized Treatment Effect	-0.008	0.035	0.066	0.592		1148

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and lottery fixed effects for eligible students. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables, except for % *school days missed*, were collected by the authors at endline.

Table B2: Treatment effect on ineligible students

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: emotional stability						
School happiness score	0.026	-0.009	0.04	0.828	0.828	3360
Self-control score	0.097	-0.067	0.044	0.126	0.377	3404
Self-esteem score	0.084	-0.066	0.047	0.161	0.241	3446
Standardized Treatment Effect	0.027	-0.062	0.046	0.183		3476
Panel B: disruptiveness						
Disruptiveness, teacher	-0.212	0.258	0.104	0.014	0.027	3203
Disruptiveness, enumerator	-0.046	0.02	0.042	0.637	0.637	3518
Standardized Treatment Effect	-0.051	0.107	0.069	0.122		4033
Panel C: academic outcomes						
% school days missed	13.089	0.331	0.742	0.656	0.656	4427
Spanish test score	0.128	-0.097	0.07	0.167	0.5	3517
Math test score	0.08	-0.035	0.065	0.589	0.884	3517
Standardized Treatment Effect	0.018	-0.038	0.058	0.515		4452
Panel D: integration in the class network						
% class friends with student	0.087	0.002	0.003	0.538	0.718	4168
Friends' average ability	0.027	-0.033	0.1	0.745	0.745	3342
Friends' average disruptiveness	-0.11	0.097	0.07	0.163	0.652	3176
No friends in the class	0.197	-0.018	0.013	0.175	0.349	4168
Standardized Treatment Effect	0.003	0.001	0.051	0.992		4171

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and lottery fixed effects for ineligible students. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables, except for *% school days missed*, were collected by the authors at endline.

Table B3: Treatment effect on classroom environment

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Disruptiveness, teacher	-0.187	0.39	0.131	0.003	0.015	160
Bullying in class, teacher	-0.038	0.062	0.159	0.698	0.698	160
Disruptiveness, enumerator	-0.186	0.389	0.148	0.009	0.021	167
Delay in class's start (minutes)	9.938	1.204	1.046	0.25	0.312	160
Average decibels during class	0.022	0.681	0.487	0.162	0.27	169
Standardized Treatment Effect	-0.215	0.424	0.131	0.001		169

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator. The regression is estimated with propensity score weights. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient, while Column (5) reports its p-value adjusted for multiple testing, following the method proposed in Benjamini and Hochberg [1995]. Finally, Column (6) reports the number of observations used in the regression. All the dependent variables were collected by the authors at endline.

Appendix C Robustness checks for classes with at least one very disruptive student

Table C1: Treatment effect in classes with at least one very disruptive students, without controls

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: Very disruptive eligible students						
Disruptiveness, teacher	0.985	-0.325	0.355	0.359	0.611	86
Disruptiveness, enumerator	0.286	0.613	0.439	0.162	0.306	85
Spanish test score	-0.460	0.038	0.335	0.910	0.910	88
Math test score	-0.230	0.063	0.512	0.902	0.958	88
% class friends with student	0.051	0.025	0.012	0.042	0.103	109
Panel B: Not very disruptive eligible students						
Disruptiveness, teacher	0.294	0.451	0.128	0.000	0.007	391
Disruptiveness, enumerator	0.162	0.103	0.127	0.417	0.644	393
Spanish test score	-0.349	-0.176	0.106	0.095	0.202	397
Math test score	-0.349	-0.092	0.167	0.581	0.823	397
Friends with ≥ 1 very dis.	0.065	0.075	0.030	0.012	0.049	397
Panel C: Ineligible students						
Disruptiveness, teacher	-0.205	0.509	0.185	0.006	0.034	1517
Disruptiveness, enumerator	-0.093	0.172	0.077	0.025	0.086	1576
Spanish test score	0.035	-0.053	0.141	0.707	0.858	1579
Math test score	0.115	-0.031	0.177	0.862	0.977	1579
Friends with ≥ 1 very dis.	0.067	0.015	0.028	0.584	0.764	1577
Panel D: Class-level outcomes						
Disruptiveness, teacher	-0.250	0.669	0.236	0.005	0.039	72
Disruptiveness, enumerator	-0.250	0.516	0.245	0.035	0.100	76

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator. To account for the fact the randomization is stratified, the regressions in Panels A, B, and C have lottery fixed effects, while in the regressions in Panel D we use propensity score reweighting. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient. Finally, Column (5) reports the number of observations used in the regression. All the dependent variables were collected by the authors at endline.

Table C2: Treatment effect in classes with at least one very disruptive students, with extra controls

Variables	Control (1)	T-C (2)	S.E. (3)	Unadj. P (4)	Adj. P (5)	N (6)
Panel A: Very disruptive eligible students						
Disruptiveness, teacher	0.985	-0.097	0.403	0.811	0.984	86
Disruptiveness, enumerator	0.286	0.226	0.500	0.652	1	85
Spanish test score	-0.460	0.109	0.354	0.759	0.992	88
Math test score	-0.230	-0.076	0.576	0.895	1	88
% class friends with student	0.051	0.011	0.018	0.544	1	109
Panel B: Not very disruptive eligible students						
Disruptiveness, teacher	0.294	0.482	0.148	0.001	0.019	391
Disruptiveness, enumerator	0.162	0.074	0.130	0.567	0.963	393
Spanish test score	-0.349	-0.196	0.101	0.052	0.146	397
Math test score	-0.349	-0.002	0.176	0.991	0.991	397
Friends with ≥ 1 very dis.	0.065	0.075	0.032	0.019	0.108	397
Panel C: Ineligible students						
Disruptiveness, teacher	-0.205	0.476	0.183	0.009	0.078	1517
Disruptiveness, enumerator	-0.093	0.122	0.082	0.136	0.331	1576
Spanish test score	0.035	0.012	0.124	0.922	0.979	1579
Math test score	0.115	0.057	0.152	0.709	1	1579
Friends with ≥ 1 very dis.	0.067	0.017	0.023	0.448	0.952	1577
Panel D: Class-level outcomes						
Disruptiveness, teacher	-0.250	0.543	0.261	0.038	0.161	72
Disruptiveness, enumerator	-0.250	0.492	0.246	0.045	0.153	76

Notes: This table reports results from OLS regressions of several dependent variables on a treatment indicator and control variables. The control variables include those selected by a Lasso regression of the dependent variable on potential controls, following Belloni et al. [2014], the variables unbalanced at baseline in the relevant subsample, and the baseline value of the outcome variable. To account for the fact the randomization is stratified, the regressions in Panels A, B, and C have lottery fixed effects, while in the regressions in Panel D we use propensity score reweighting. Column (1) reports the mean of the outcome variable for the control group. Column (2) reports the coefficient of the treatment indicator. Column (3) reports the standard error of this coefficient, clustered at the lottery level. Column (4) reports the unadjusted p-value of this coefficient. Finally, Column (5) reports the number of observations used in the regression. All the dependent variables were collected by the authors at endline.